Shared Premises, Different Conclusions

David C. Palmer
Smith College

John W. Donahoe
University of Massachusetts/Amherst

With respect to the position of Baron, Perone, and Galizio regarding the suitability of human subjects in conventional operant research, we find ourselves in the somewhat paradoxical position of agreeing with most of their arguments, but drawing opposite conclusions. Certainly we share the authors' assumption that a common set of principles describes human and nonhuman behavior, and no one will be more delighted with any demonstrated success in the use of human subjects in basic operant research. However, we remain skeptical that the use of human subjects will lead to new insights into basic behavioral processes or the principles that summarize their actions. In our view, our most fruitful course will involve vigorous efforts to clarify remaining ambiguities in basic behavioral principles in the animal laboratory (e.g., Donahoe, Crowley, Millard, & Stickney, 1982; Stickney & Donahoe, 1983) coupled with equally vigorous interpretations of complex phenomena (e.g., Donahoe, 1991; Donahoe & Palmer, 1989; Palmer, 1991).

There are at least three reasons why someone would wish to do basic research with humans: (a) to demonstrate that a common set of principles governs both human and nonhuman behavior; (b) to study species differences, that is, to identify or explore phenomena unique to humans; (c) to refine or extend our understanding of basic behavioral principles. It is evident that the first two goals can be pursued only with human subjects, whatever the methodological difficulties.

Address correspondence to either author: David C. Palmer, Department of Psychology, Smith College, Northampton, MA 01063; John W. Donahoe, Department of Psychology, University of Massachusetts, Amherst, MA 01003.

As for the third goal, however, human subjects are unsuitable, for all the reasons cited by Baron et al., as well as for some not mentioned. If the principles under study are general, there appears to be no reason to use human subjects and many reasons why one would wish to avoid doing so.

Demonstrating that behavioral principles apply to humans as well as to other organisms is a worthy goal, but for primarily polemical reasons. So long as work carried out within our research tradition suggests that even relatively simple examples of human behavior are not well described by these principles, it will be awkward persuading skeptics that our interpretations of complex behavior are plausible. However, it is unlikely, in our view, that such research would be either conclusive or persuasive in the absence of the level of control possible in the animal laboratory. If our results are inconsistent with animal work, we could conclude either that different processes are at work or that we have failed to control crucial variables. On the other hand, if our results are consistent with animal work, one might argue that the results are the same for different reasons and that under other conditions we would find discrepancies. It would be easy for the skeptic to find empirical support for this latter view. Thus, while we can have no objections to someone pursuing research of this sort, we question whether such research will fulfill its purpose. Moreover, even if successful, it will not advance our formulation of basic principles; it will serve mainly to shore up the applications and interpretations that have taken for granted the generality of basic principles.

Studying species differences will always be an important goal. Humans are

obviously unique—as are all species—and any clarification of our uniqueness would be a welcome and important contribution. Are the events that can serve as primary reinforcers different, in part, for humans than for other organisms? Do humans differ in cross-modal generalization, or in the extent to which incipient responses can come under stimulus control, or in the potential number of environment-behavior relations affected by a single instance of reinforcement? Are there human analogs of the differential associability of stimulus modalities and response systems found in pigeons and rats? Answers to such questions will greatly extend our understanding of human behavior; however, the methodological and ethical obstacles to this kind of research seem to us to be far more formidable than those facing the researcher who merely attempts to show the generality of the reinforcement principle.

Baron et al. raise three objections to such skepticism. First, they argue that historical variables are important in nonhuman research as well as in human research, and they cite the research of Hebb (1949) and Thomas (1969) to illustrate the role of pre-experimental experience in animal research. While the point that historical variables cannot be eliminated is well taken, it is surely desirable to control as many as possible. With respect to the control of both phylogenetic and ontogenetic variables, nonhuman subjects are preferable. Note that the illuminating experiments of Hebb and Thomas could not be done with humans.

Secondly, the authors point out that there are techniques for attempting to control historical variables with human subjects, including steady-state designs, long-term observations, and subject selection. These are excellent suggestions, but it remains to be shown that these techniques are equal to the task of providing adequate control with human subjects. Ultimately it is an empirical question, of course, but we have been impressed with the difficulty of analyzing contingencies in the animal laboratory that are just slightly more complicated

than the classic preparations. We find ourselves perhaps as physicists found themselves 300 years ago. Newton observed that his principles of motion quite handily accounted for the orbit of the moon around the earth or the orbit of the earth around the sun. However, he found it no easy matter to account for the motion of the three bodies together, though he worked on it many years. (It was the only problem, he claimed, that made his head ache.) It was later attacked by both Euler and Gauss with no better success. Only with the advent of powerful analog computers has it been possible to program the three-body problem in full generality.

Similarly, the introduction of merely a second discriminative stimulus or a second operandum greatly complicates the task of predicting and controlling the behavior of a pigeon, as a generation of research on the matching law has shown. In our own laboratory, in an investigation of blocking in the pigeon, we found that extensive parametric research was necessary to identify two equipotent stimuli (cf. Foree & LoLordo, 1973; Randich, Klein, & LoLordo, 1978); even so, very slight individual differences in responding to the stimuli caused us trouble (Palmer, 1988; described in Donahoe, Burgos, & Palmer, in press). How much more difficult would it be to interpret the behavior of a human subject responding under the control of many implicit concurrent schedules in environments in which many stimuli are meaningful (i.e., discriminated)?

All contingencies are on concurrent schedules, whether in the laboratory or not. The perturbing influence of concurrent contingencies is diminished in the animal laboratory by ensuring that one contingency is extremely powerful while all others are relatively weak; specifically, we deprive our animals of food or water to ensure a relatively stable and powerful contingency. In the human laboratory it is seldom possible, for reasons of ethics or economy, to establish such a dominant contingency. If behavior under the nominal contingency appears to contradict established principles, it may be because of

the many concurrent schedules that we have not controlled. Pigeons and rats neither know nor care that their responses are being observed by researchers, but it is a rare human who does not suffer some "evaluation anxiety" in an experimental setting. Are points or coins reinforcing to such subjects, or do they continue to perform to escape our censure? How many of our human subjects would continue to work if we wheeled our experimental apparatus into the hall and went home? If our nominal reinforcers are, in fact, reinforcing, do they remain equally effective throughout the experimental session, or from one session to the next, or from one subject to another? These questions, among others, suggest that many experiments with humans are not just a bit more complicated than experiments with nonhumans; they are vastly more complicated. We should note, however, that these difficulties are not confined to our field. All human research suffers from the same limitations; they cannot be avoided by fleeing to a more permissive paradigm such as cognitive psychology. To the contrary, we are especially well placed to investigate human behavior, as we have both a proven methodology and a set of basic principles to guide our inquiry.

The third objection of Baron et al. is that interpretation and application cannot substitute for experimental analysis in demonstrating the generality of behavioral principles. This is true, but may be an inescapable dilemma. Again, we cannot quarrel with the proposal that behavioral principles should be demonstrated in humans as far as possible, but we expect that complex human behavior will remain beyond the reach of an experimental analysis for some time to come—perhaps indefinitely. Moreover, we think that interpretation has a much larger role to play than it has hitherto. It is characteristic of historical sciences, such as evolutionary biology, cosmology, and behavior analysis, that much of the domain is beyond the scope of experimental analysis; we must rely on interpretation for our understanding of phenomena. It is common to suppose that interpretation is a poor cousin to experimental analysis, something to which we resort because we have nothing better to offer. To the contrary, considering the scope of the two enterprises, experimental analysis is better viewed as the handmaiden of interpretation; we engage in experimental analysis so that we can interpret the world. Our understanding of nature would be slight indeed if it were confined to those phenomena that have been analyzed experimentally. Most of our scientific understanding of the world is interpretation: No one has done an experimental analysis of the tides or of the orbit of planets or of the evolution of the wing, and most of our everyday explanations for the way things work are interpretations, albeit often straightforward ones, based on a few well established physical principles.

In behavior analysis, as in evolutionary biology, interpretation is particularly important, because our ability to interpret human behavior far outstrips our ability to analyze it experimentally. The adequacy of the behavioral viewpoint rests on the scope of its interpretations and the extent to which the interpretations indeed follow from the principles identified in experimental analyses. Our field suffers, not from too much, but from insufficient interpretation. Skinner's analyses of complex behavior are brilliant, but they merely laid groundwork for others to build upon. In our view, much remains to be done in the domains of perception, memory, and verbal behavior.

As we have argued elsewhere (Donahoe & Palmer, 1989), verbal interpretations are often of limited scope; they should be supplemented whenever possible by organismic interpretations (e.g., the "Columban simulations" of Epstein, Skinner, and others) and by formal interpretations (computer modeling and the adaptive network research now much in vogue in cognitive science). Formal interpretations have two advantages: They enable us to analyze phenomena too complex for verbal interpretation (e.g., Epstein, 1985), and they are typically persuasive. We have been struck by the

excitement aroused among researchers in adaptive networks by findings that are no more than formal equivalents of statements that Skinner had been making for half a century. We believe that adaptive network research that explicitly implements behavioral principles will be particularly successful and may serve to hasten the rediscovery of radical behaviorism by the emerging generation of cognitive scientists. Formal interpretations, we suspect, will persuade the skeptics as surely as laboratory demonstrations will, that behavioral principles can be extended to humans.

Our position is by no means inconsistent with that of Baron et al. It is true that we expect a relatively larger contribution from interpretation and a relatively smaller one from the experimental analysis of human behavior than they, but we acknowledge that some research with humans is necessary. Moreover, we applaud their suggestion that the traditional procedures of the operant laboratory be employed in such research. However, we would caution against doing research that displays only the superficial trappings of the animal laboratory; we should go back to our experimental roots for our methodology. Skinner (1935, 1938) pointed out the need for empirically identifying our units of analysis, for both stimuli and responses. Long experience and widely replicable results have established key-pecking, bar-pressing, lights, tones, and food pellets as appropriate units of analysis under many conditions, so that this preliminary work is often omitted in the animal laboratory. Many researchers take for granted that the contingency as defined by the experimenter is the contingency controlling the behavior of the organism, and behavior is frequently analyzed without concern for the validity of its units. However, even in the animal laboratory this is often a mistake, as we have found (Palmer, Donahoe, & Crowley, 1985). In far more loosely controlled human studies it is almost certain to cause trouble. Much human research in other fields of behavioral science is, in our view, simply uninterpretable because of lack of attention to such details. We would be well advised to heed the counsel of Baron et al., and exploit the methodology of the operant laboratory that has served us so well in the analysis of the behavior of nonhuman organisms.

REFERENCES

Donahoe, J. W. (1991). The selectionist approach to verbal behavior: Potential contributions of neuropsychology and connectionism. In L. J. Hayes & P. N. Chase (Eds.), Dialogues on verbal behavior (pp. 119–145). Reno, NV: Context Press.

Donahoe, J. W., Burgos, J. E., & Palmer, D. C. (in press). A selectionist approach to reinforcement. In M. L. Commons, J. W. Donahoe, & A. Kacelnik (Eds.), Quantitative analyses of behavior: Vol. 16. The nature of reinforcement. Hillsdale, NJ: Lawrence Erlbaum.

Donahoe, J. W., Crowley, M. A., Millard, W. J., & Stickney, K. A. (1982). A unified principle of reinforcement. In M. L. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), Quantitative analyses of behavior: Vol. 2. Matching and maximizing accounts (pp. 493–521). Cambridge, MA: Balinger.

Donahoe, J. W., & Palmer, D. C. (1989). The interpretation of complex human behavior: Some reactions to Parallel distributed processing, edited by J. L. McClelland, D. E. Rumelhart, and the PDP Research Group. Journal of the Experimental Analysis of Behavior, 51, 399–416.

Epstein, R. (1985). Animal cognition as the praxist views it. Neuroscience and Biobehavioral Reviews, 9, 623–630.

Foree, D. D., & LoLordo, V. M. (1973). Attention in the pigeon: The differential effect of food getting vs. shock avoidance procedures. Journal of Comparative and Physiological Psychology, 85, 551–558.

Hebb, D. O. (1949). The organization of behavior: A neuropsychological theory. New York: Wiley.

Palmer, D. C. (1988). The blocking of conditioned reinforcement. Unpublished doctoral dissertation, University of Massachusetts.

Palmer, D. C. (1991). A behavioral interpretation of memory. In L. J. Hayes & P. N. Chase (Eds.), Dialogues on verbal behavior (pp. 261–279). Reno, NV: Context Press.

Palmer, D. C., Donahoe, J. W., & Crowley, M. A. (1985). Discriminated interresponse times: Role of autoshaped responses. Journal of the Experimental Analysis of Behavior, 44, 301–313.

Randich, A., Klein, R. M., & LoLordo, V. M. (1978). Visual dominance in the pigeon. Journal of the Experimental Analysis of Behavior, 30, 129–137.

Skinner, B. F. (1935). The generic nature of the concepts of stimulus and response. Journal of General Psychology, 12, 40–65.

Skinner, B. F. (1938). The behavior of organisms. New York: Appleton-Century.

Stickney, K. J., & Donahoe, J. W. (1983). Atten-

uation of blocking by a change in US locus. *Animal Learning and Behavior, 11,* 60–66.

Thomas, D. (1969). The use of operant conditioning techniques to investigate perceptual processes in animals. In R. M. Gilbert & N. S. Sutherland (Eds.), *Animal discrimination learning* (pp. 1–33). New York: Academic Press.